

THURSDAY, OCTOBER 11, 1883

THE METAPHYSICAL FOUNDATIONS OF  
NATURAL SCIENCE

*Kant's Prolegomena and Metaphysical Foundations of Natural Science.* Translated, with a Biography and Introduction, by Ernest Belfort Bax. (London: George Bell and Sons, 1883.)

THE pages of NATURE are not the appropriate place for the review of works on general metaphysics. The genius and methods of science are so different from those of philosophy that, as their respective histories have amply shown, these branches of intellectual activity are as a rule best kept asunder. But there is at least one important point of contact which cannot be overlooked. And it is just because in the writings of Kant, and particularly in the second of the two treatises which are translated in this volume, that alleged point of contact was formulated for the first time that his work rightly or wrongly demands notice in these pages. As regards the translation, Mr. Bax has done his work with care. He has undertaken simply to furnish a literal and accurate translation of the "Prolegomena" and "Metaphysische Anfangsgründe," and he has fulfilled his undertaking. We should have been glad could he have seen his way to banish such inelegant and inaccurate renderings of "Vorstellung" and "Anschauung" as "representation" and "intuition," and to substitute for them "idea" and "perception," which, despite their vagueness, are English words of intelligible significance. But no one can fail to find in the translation, as it stands, a faithful and consistent rendering of the original.

In an essay on the relation of philosophy to science contributed to a volume entitled "Essays in Philosophical Criticism," I had recently occasion, in conjunction with my brother, to formulate in some detail what are conceived to be not merely Kant's own criticisms of the subject, but certain definite results obtained by the application of the Kantian analysis of the nature of knowledge to some of the methods of science. I mention this circumstance because that essay has undergone vigorous criticism at the hands of Mr. Romanes in a review which appeared in NATURE of August 23 (p. 386), and because the concise and definite objections taken by him *in limine* to the title of the theory of knowledge to criticise certain of the leading scientific conceptions, form a point of application for a review of Kant's teaching.

In the first place it is necessary to state at the outset what the somewhat increasing number of people who read Kant intelligently think about science. That science has justly dominated the region of knowledge generally is for them a truism, and they repudiate in emphatic language any attempt to speculate by *a priori* reasoning upon matters which fall within the province of observation and experiment. Whenever there occurs a question which is really one of fact in nature, that question they recognise as for science alone. But then they say that it is not the faith but the scepticism of men of science which is too small. They ask men of science to consider their general conceptions—to criticise their categories—a little more than they have been in the habit of doing.

VOL XXVIII.—No. 728

They doubt whether such questions as the common one whether the phenomena denoted by the word "life" came into existence at a particular time as the effect or creation of some cause or conditions (whether inorganic or supernatural is irrelevant) are really questions of fact. They profess to be able to show that the dilemmas raised in such cases are the result of the application of conceptions which have really no application, and that such dilemmas have as little foundation as that which arises when, to refer to Mr. Romanes' illustration, we ask whether a piece of mechanism is comical or not comical. It is alleged to be the achievement of Kant to have shown that such questions as we have indicated are irrational and absurd, and that their existence necessitates on the part of men of science the possession of philosophical knowledge. Of this class of questions there may be mentioned by way of illustration the general problems of the commencement or non-commencement of the universe or of life in time, and of the existence of an absolutely First Cause, and the validity of a multitude of assumptions in our inquiries into the facts of nature which pass more or less unquestioned.

In the article already referred to, Mr. Romanes peremptorily refuses to accept the result that it is impossible to regard biological phenomena as the effect of mechanical causes, or, more accurately, to find in experience a case of *abiogenesis*. "It is," he says, "the worst form of dogmatism thus to affirm on grounds of metaphysical speculation alone the antecedent impossibility of any discovery in science, most of all with reference to a matter touching which we are so much in the dark." Now this "demurrer to the relevancy" is an *ignoratio elenchi*. Such a question is for Kant not one of discovery in science at all, but a false issue, which discloses its unintelligible and absurd nature whenever we ask ourselves the preliminary question, what is meant by organisation and mechanism. Let us examine more closely the point made by Mr. Romanes. The living organism is derived from one more simple, and the latter from one yet more simple, the process extending back without an apparent limit. Therefore, says Mr. Romanes, it is unscientific to deny the possibility that there may be a case of organisation so simple that it will be seen to be a mere mechanical arrangement. But the series in like manner tends to reach its limit and the curve to touch the asymptotic line, and yet it is neither unscientific nor unwarrantable *a priori* reasoning to show that a coincidence will never be found in experience. We learn what we have here by defining what is involved in the nature of the series and the limit of the curve. Nobody wishes to deny that organisation and the present state of the world generally may have been attained by a process of evolution from a mass of gaseous vapour. What is denied is that it is the same thing or other than an unintelligible statement, to say that organisation is or may have been evolved out of a mere mechanical arrangement. There is a great distinction between these propositions. Science is a process of abstraction in which attention is concentrated on a certain kind or category of relation to the exclusion of other kinds. For instance, in physical science we look only at those dynamical and statical relations which are expressed in time and space, such as causation and reciprocity. Again, in biology

B B

we have before us the facts of organisation and development. But it is one thing to consider a single set of relations, such as those of causation, to the exclusion of the rest, for the sake of clearness of knowledge, and quite another to say that this particular aspect of the object exclusively constitutes it. Mr. Romanes thinks that biologists do not require any transcendental analytic to inform them that an organism is something more than a mechanism. But he finds it startling to be told that in the investigation of an organism we are to rise above the category of causality, and carry into our inquiry the conception of teleology. Surely the latter proposition is the logical consequence of the former. No one says that the category of causation is not to be used in the investigation of the phenomena of organisation. In anatomy, and in its dynamical correlative physiology, the parts of the organism are constantly treated as independent of each other, and related as cause and effect. But this is an abstract point of view employed for a special purpose—the obtaining of measurements—and is qualified by the recognition of the complete conception of the organs as part of a self-conserving whole or system. This is all that is implied by the unfortunate term “teleology” when used in the theory of knowledge. What Kant professes to show is that this fact of nature cannot be reduced to or expressed in terms of the dynamical and statical relations of time and space. No doubt the laws of matter and energy apply in biology as strictly as elsewhere, but they do not express, much less exhaust, biological phenomena. And therefore we must be careful in biology not to distort those conceptions or hypotheses which are, despite assertions to the contrary, the necessary guides and interpreters of observation and experiments by the exclusive employment of categories which, like causation, neither are drawn from, nor are adequate to, the facts. The subject of the detailed effect of the neglect in this reference of Kant’s warning I will not pursue here, as my brother has treated it at some length, with special reference to the objections made by Mr. Romanes, in a paper which will appear elsewhere. It ought to be borne in mind, as illustrating the point of view here emphasised, that Kant himself was one of the first to advance the nebular hypothesis. The truth is that, in speaking of the universe as having presumably originated from a mass of incandescent vapour, Kant, and everybody else, so far from reducing life to mechanism, is really raising mechanism to life. Kant would have told us that in the phenomena of such a developing mass there were potentially present all the relations of the universe as we know it. No doubt the approximate conceptions for the advance of knowledge are at this point the laws of matter and energy. But these do not exhaust the object, and if we have abstracted from the others we have done so in just the same way as we have abstracted from fact that the phenomena are there only for a percipient subject.

Such considerations and the doubts they raise may seem remote. But the number of those is increasing who think that they should be better known to and understood by men of science. It will not do to say that such criticism has no bearing on scientific inquiry until it has been ascertained whether its neglect has not already—even in matters of minute detail—misled and stultified certain

phases of such inquiry. Fact and theory are not so very easy to distinguish. With scientific method no one wishes to interfere. But we would subject to closer investigation the question whether what are commonly taken to be the legitimate problems of science are really what they profess to be. It is not to the “Hannibals” of science, but to her Don Quixotes that Kant addresses himself.

R. B. HALDANE

## LETTERS TO THE EDITOR

*[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]*

*[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]*

### The “Transmission Eastwards Round the Globe of Barometric Abnormal Movements”

THE following questions suggest themselves in connection with the above matter:—(1) Is it a fact that atmospheric movements of such small amplitude take the form of waves, and, if so, that the waves have so slow a rate of travel? (2) How is it to be accounted for that the waves travel eastward more slowly than westward? And (3) How can it be explained that they appear at an eastward station with a greater amplitude than at a westward?

With regard to the first question, it may be said that almost absolute proof of the existence of such waves can be brought forward. A recent investigation, the results of which are not yet published, has shown that a barometric wave measuring from maximum to minimum only  $\cdot 108$  inch, which occurred in Western India in 1877–78, was accurately reproduced over the same region three years later, namely, in 1880–81. The wave at the time of its reappearance had all the larger details which it possessed during its original appearance, these details agreeing in many cases to within less than  $\cdot 001$  inch. But whereas the amplitude in 1877–78 was  $\cdot 108$  inch, in 1880–81 it was only  $\cdot 048$  inch. Waves have also been recognised which appear in the summer half of the year at the northern part of Western India and travel southwards, arriving at the southern parts between two and three months after their appearance in the north; and also waves which appear in the winter half of the year at the south and travel northwards. This movement from north to south and from south to north during alternate halves of the year has been traced regularly since 1869; and indeed is so constant that in many cases it has been possible by means of it to calculate quantitatively the average position of the barometer during the next three months.

The second question was answered in my paper which you were good enough to publish in your issues of the 9th and 16th ult. Owing to the upper air currents travelling from equatorial to higher latitudes, and the lower air currents travelling equatorwards, there must be in high latitudes a general movement of the atmosphere eastwards, whereas in tropical and subtropical regions there must be a general movement westwards. This at once explains why in tropical and subtropical regions the atmospheric waves should travel more rapidly westwards than eastwards.

The third question is a difficult one, and the answer not perhaps quite satisfactory. If it were the case that undulations in fluid bodies become heaped up and increased in amplitude when travelling in a direction opposite to that of a current, and are affected in a contrary way when travelling with the current, a satisfactory answer might be furnished.

But an explanation may be sought in a different direction. If the circumstances of any latitude situated a little way from the equator be considered, it will be noticed that two principal air currents are flowing there—a lower one with a westward component of a certain velocity, and an upper one with an eastward component of a less velocity. Now it may be supposed that these two currents are affected by waves of two kinds, the first being waves common to the two currents, the second being waves which have been generated in the two currents in regions from which they are proceeding. And there are two reasons